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Response to Tarrach's "Mode Dependent Field Renormalization and Triviality"

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Abstract:

We respond to Tarrach's criticisms of our work on $\lambda\Phi^4$ theory. Tarrach does not discuss the same renormalization procedure that we do. He also relies on results from perturbation theory that are not valid. There is no "infrared divergence" or unphysical behaviour associated with the zero-momentum limit of our effective action.

In a recent paper Tarrach [1] has criticized our work on $(\lambda\Phi^4)_4$ theory in which we obtain a "trivial" but not entirely trivial continuum limit [2]. However, (**1**) Tarrach does not consider the same renormalization procedure that we do, and thus his "main result" (Eq. (23)) has no relevance to our proposal; (**2**) his discussion assumes results from perturbation theory that are not valid; and (**3**) his implication that there is something physically pathological about the zero-momentum limit of our effective action is not true.

1. Although comparison is somewhat obscured by Tarrach's very different terminology and notation, there is an easy way to see that he is discussing a quite different renormalization procedure from ours. We both consider a re-scaling of the zero-momentum mode of the field, and hence of its vacuum value v, but Tarrach's is different from ours. In our work the key requirement is that the combination $\lambda_B v_B^2$, governing the physical mass, should be finite. In our notation λ_B is the bare coupling constant, which tends to zero like $1/\ln(\text{cutoff})$, and the finite, physical v is related to the bare field by

$$v_B = Z_\phi^{1/2} v \tag{1}$$

with $Z_\phi \sim \ln(\text{cutoff})$, so that $1/Z_\phi$ *scales like* λ. In Tarrach's paper the corresponding equation is in the last line of Eq. (20):

$$\text{``}v_R = Z_A^{-1/2} A\text{''}, \tag{2}$$

where "v_R" is essentially our v_B (it is "$Z_R^{-1/2} v_B$" with "Z_R" ~ 1) and "A" is the finite quantity (our v). Thus, Tarrach's "Z_A" is $1/Z_\phi$. However, *it does not scale like* λ: in his continuum limit ("$\tau \to 0$"), it scales as $|\ln\tau|^{-1/2}$ (his Eq. (21)) while λ scales as $|\ln\tau|^{-1}$ (his Eq. (19)). Thus, Tarrach's renormalization is not ours. The fact that he finds no surviving mass term in his renormalized effective action (Eq. (23)) is unsurprising, and has no bearing on our work.

Though, for reasons to be explained below, we do not accept Tarrach's initial premise, Eq. (17), it might be instructive to point out that he could have produced a more accurate caricature of our picture by replacing his postulated Eq. (18) with

$$a \sim \tau^{1/2}\,|\ln\tau|^{-1/6}\,L. \tag{3}$$

This would yield our re-scaling for v and also an "m_R" that is finite in physical units. Superficially, it leads to an effective potential that is of order $\ln(\text{cutoff})$, but in our picture, as originally in Ref. [3], this is remedied by a cancellation. This cancellation is simply the fact that a function made up of a log-divergent ϕ^4 term and a finite $\phi^4 \ln\phi^2$ term can always be re-written as $\phi^4(\ln \phi^2/v^2 - \frac{1}{2})$, with the divergence absorbed into the vacuum value v.

2. Tarrach's starting point, his Eq. (17), relies on results from renormalization-group-improved perturbation theory (RGIPT). He claims that these results are "very solidly founded, because RGIPT is, at low energies, and *because of triviality* [our italics], very reliable." This is a common misconception: It falsely assumes that a small (or vanishingly small) renormalized coupling is a sufficient condition for RGIPT to work. In fact, the traditional approach and "triviality" are inherently contradictory about the continuum limit; the former begins by postulating a finite, *non-zero* renormalized coupling constant, and "triviality" says that there can be no such thing.

In [4] we discuss exactly what goes wrong with RGIPT: Its re-summation of leading logs tries to re-sum a geometric series that is inevitably *divergent* when one tries to take the continuum limit. Our not-entirely-trivial continuum limit arises precisely where the leading-log series becomes $1-1+1-\ldots$, which RGIPT assumes will re-sum to $1/(1+1) = 1/2$. There are instances in physics where such an illegal re-summation happens to give the right answer — but this is not one of them.

Tarrach's Eq. (17) assumes, based on perturbation theory, that spontaneous symmetry breaking (SSB) in lattice $(\lambda\Phi^4)_4$ theory corresponds to a *second-order* phase transition. This is not true in our picture, and recent lattice data [5] strongly supports our claim. *A priori*, for a given value of the bare coupling constant, λ_B, one can define two distinct critical values of the bare-mass-squared parameter $r \equiv m_B^2$; one, r_{PhT}, is where the phase transition actually occurs; the other, r_{CSI}, is where the mass gap of the symmetric phase becomes exactly zero (the "classically scale-invariant" (CSI) case). If these two values exactly coincide then the transition is second order. If that were so, then a continuum limit could be obtained for *any* λ_B by taking the limit $\tau \to 0$, where $\tau = |1 - \frac{r}{r_{\mathrm{CSI}}}|)$, since the physical correlation length would then diverge in units of the lattice spacing.

However, to find out whether r_{CSI} and r_{PhT} coincide, one must explore the effective potential of the theory. As discussed in our papers [2], in any approximation consistent with "triviality" — i.e., one in which the shifted field $h(x) = \Phi(x) - \langle\Phi\rangle$ is effectively governed by a quadratic Hamiltonian, with its propagator determined by solving exactly a non-perturbative gap equation — the massless theory at $r = r_{\mathrm{CSI}}$ lies within the broken phase; i.e., r_{CSI} is more negative than r_{PhT}. Our approach predicts that the exact form of the effective potential in the CSI case is $\phi^4(\ln \phi^2/v^2 - \frac{1}{2})$, and this has been confirmed to great accuracy by lattice simulations [5].

Since $r_{\rm CSI}$ and $r_{\rm PhT}$ differ, the phase transition is first-order. In order to obtain a continuum limit, one needs the physical correlation length ξ_h of the *broken* phase to be infinite in units of the lattice spacing. In other words, the mass $m_h \sim 1/\xi_h$ of the fluctuations about the SSB vacuum must be much, much less than the cutoff. As discussed in our papers [2, 5], this requires λ_B to tend to zero like $1/\ln(\text{cutoff})$ [2].

With such a λ_B, although $r_{\rm CSI}$ and $r_{\rm PhT}$ differ, they differ – even in physical units – only by an infinitesimal amount: each is negative and huge, of order $(\text{cutoff})^2$, while their difference is infinitesimal, of order $1/\ln(\text{cutoff})$. However, all the interesting physics occurs over such an infinitesimal range of r around $r_{\rm PhT}$. This is because such tiny variations in r cause *finite* changes (i) in the particle mass of the broken vacuum, (ii) in the energy-density difference between the two phases, and (iii) in the barrier between them. The problem with the conventional approach is that it looks at the phase transition on too coarse a scale — making finite variations in r. Viewed on that scale the transition appears indistinguishable from a second-order transition and the not-entirely-trivial physics is not seen.

3. Tarrach also alleges that our effective action is "infrared divergent." It is not clear what he means by this. There is, of course, the usual infinite-volume factor in the relation between the effective action and the effective potential. In a derivative expansion of the effective action the term with no derivatives is $-\int d^4x\, V_{\rm eff}(\Phi(x))$, so that if $\Phi(x) = \phi = \text{constant}$ one gets $-(\int d^4x)\, V_{\rm eff}(\phi)$ (see, eg. [6]). Physically, this is natural — the energy diverges with the volume if the energy density is finite — but it is rather improper mathematics. Tarrach objects to having a *constant* source, and hence a constant ϕ, $\neq v$, insisting that all sources should fall off to zero at infinity. However, that is only one way of regularizing. More conveniently the theory can be formulated in finite volume with periodic boundary conditions; there is then no problem with considering a source that is constant over this volume. An excellent treatment of our picture in a finite-volume formalism has been given by Ritschel [7]. This issue has nothing to do with our non-traditional ultraviolet renormalization.

It is true that our renormalized effective action is discontinuous at zero momentum, in that the renormalized proper n-point functions ($n \geq 3$) are zero at finite momentum, but are non-zero at zero momentum. [Our renormalized 2-point function, however, has *no* discontinuity at zero momentum; our field renormalization is precisely what is needed to ensure this.] However, this discontinuity could never be directly revealed experimentally, because scattering experiments with exactly zero-momentum particles are inherently impossible. Moreover, S-matrix elements are more directly related, not to the proper Green's functions generated by the effective action, but to the *full* Green's functions. The latter are inherently singular at $p = 0$, whenever there is SSB, because they contain disconnected pieces proportional to $\delta^{(4)}(p)$. Smoothness at $p \to 0$ is not to be expected since the underlying phenomenon is Bose condensation. Macroscopic occupation of the $p = 0$ mode gives it a unique status, making it entirely natural that it requires its own special re-scaling.

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